THE CHEMICAL HERITAGE FOUNDATION

CHARLES D. HURD

Transcript of an Interview
Conducted by

James J. Bohning

in

Evanston, Illinois

on

28 February 1991

With Subsequent Additions and Corrections

THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY
Oral History Program

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(Signature) Charles D. Hurd
Dr. Charles D. Hurd

(Date) Oct 18, 1991

(Revised 20 February 1989)

CHARLES DEWITT HURD

1897 Born in Utica, New York on 7 May

Education

1918 B.S., chemistry, Syracuse University
1921 Ph.D., organic chemistry, Princeton University

Professional Experience

1917 Thomas A. Edison, West Orange, New Jersey
1918 Chemical Service Section, United States Army
1921-1924 Instructor, chemistry department, University of Illinois

Northwestern University, chemistry department
1924-1928 Assistant Professor
1928-1933 Associate Professor
1933-1949 Professor
1949-1951 Morrison Professor
1951-1965 Clare Hamilton Hall Research Professor 1965-Clare Hamilton Hall Research Professor, Emeritus

Honors

1943 Sc.D., honorary, Syracuse University
1958 Midwest Award, American Chemical Society, St. Louis Section
1971 Austin M. Patterson Award, American Chemical Society
1974 Honorary membership, Illinois State Academy of Science
1978 Distinguished Service Award, American Chemical Society, Chicago Section

ABSTRACT

Charles Hurd begins the interview with information and anecdotes about his childhood in upstate New York and his stepfather's career in administering boarding schools and colleges. He discusses his undergraduate education at Syracuse University, his research during World War I in the Chemical Service Sector on poison gas, and a summer job working with Thomas Edison for the Naval Consulting Board. Hurd then describes his graduate work at the University of Minnesota and Princeton University, during which he began his work on ketenes and pyrolysis. This work continued during his instructorship at the University of Illinois, which culminated when Hurd was recruited to Northwestern University by Frank Whitmore. Hurd describes his career at Northwestern and discusses his interests in nomenclature, his work on sugar chemistry and his numerous consultancies in industry. Hurd names many of the graduate students with whom he worked and describes the dynamics of his relationship to them as mentor and his introduction of "Molecular Models" as a teaching tool. Hurd discusses the media's "chemophobia" and negative portrayal of the chemical industry, and describes his own published efforts to dispel this negative image and his writings for encyclopedias and dictionaries. Hurd concludes the interview by briefly examining other research projects, patents and colleagues.

INTERVIEWER

James J. Bohning, Assistant Director for Oral History at the Chemical Heritage Foundation, holds the B.S., M.S., and Ph.D. degrees in chemistry. He was a member of the chemistry faculty at Wilkes University from 1959 until 1990, where he served as chair of the Chemistry Department for sixteen years, and chair of the Earth and Environmental Sciences Department for three years. He was Chair of the Division of the History of Chemistry of the American Chemical Society in 1987, and has been associated with the development and management of the Center's oral history program since 1985.

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INTERVIEWER: James J. Bohning

LOCATION: Evanston, Illinois

DATE: 28 February 1991

BOHNING: With me are Dr. Alfred Bader and his wife Isabel. Dr. Hurd, I know you were born on May 7, 1897 in Utica. Could you tell me something about your parents and your family background?

HURD: My father was born in 1850 and he had a first wife with three boys. She died and he married my mother, who was born in 1875, 25 years younger than he was. My mother married him in 1892. They had two children; I have an older sister who was born in 1895. He was a jobber of shoes. He had a retail shoe store in Utica but wholesale within the distance that they could reach. In those days there were no automobiles, so they used trains and things. I imagine that he met my mother on one of those wholesale trips, because her home was in Colosse, New York. You've never heard of it. You probably haven't ever heard of Parish or Mexico, either. Well, a map would help. [A map is retrieved.] Here's Utica over here, here is Syracuse, and twenty-five miles up from there is Mexico and Parish. Can you locate them?

BOHNING: Yes.

HURD: It's about fifteen miles east of Oswego. Colosse is right between those two towns. It's only a crossroad farming community, but that's where she was born. She got married, as I indicated, and had the two kids. Then he died of typhoid fever before I was a year old. Well, she wasn't able to handle those stepsons. The oldest one was only a few years younger than she was. So she went back to Colosse. To help out paying the bills, she had to eke out a job teaching in the grade schools in Mexico. Her husband had already died, so I didn't ever know my father at all. While she was alive, I was stupid not to ask her quite a lot about him. But I didn't, so I don't know anything except that he had a home in Utica and he sold shoes. He was the sixth of eight children. The oldest one was named De Witt and my middle name is named after the oldest one. De Witt was named after the governor of New York State that built the Erie Canal, namely De Witt Clinton. In other words, these days they sort of imitate names just as they did in the old days. But I was named after him. He was also in shoes. If they hadn't died, I'd probably be in shoes. [laughter] Who knows.

In Mexico the high school and the grade school were all under the same roof in a pretty big building. My mother met the principal, whose name was Arthur H. Norton. And he really was my father. They married when I was age five, and lived in Mexico for a few years until 1904, when he got a call to be principal of Cook Academy in Montour Falls, New York. Now, Montour Falls is the new name of that town. It had been named Havana, but there was a Colonel Cook who was sort of a politician in the town and he didn't like the name. He's the one that built Cook Academy and could cause almost anything he wanted to happen. He disliked the name Havana so he picked out the name Montour Falls. There's no falls in the world that is called Montour Falls, just the town. That area has a bunch of waterfalls and glens. Three miles to the north of Montour Falls is Watkins Glen. That's at the south end of Seneca Lake, which is really a tremendous lake. It's one of the deepest lakes in the whole continental United States, only three miles wide and 40 miles long, but about 600 feet deep. Well, there was a Watkins Glen, but there still is a Havana Glen. In 1904 you paid ten cents if you wanted to go and see either one. Now, ten cents would buy two loaves of bread. That's better than you can buy one loaf for now at a dollar and a half. That tells you that that was a high price if you wanted to go and see either Havana or Watkins Glen.

BADER: Your sister was with you. You had an older sister.

HURD: An older sister, that's right. After my mother married A. H. Norton, I got two half-sisters that lived with me. I don't think those half-sisters ever met my three half-brothers. They were all in Utica and I wasn't. Now, Mother was totally French. Her grandparents were French immigrants and her father was a French immigrant. English is something that he never learned. So French was the language of the home in Colosse, New York. She had to learn English when she went to the first grade.

BADER: French or French Canadian?

HURD: No, French from France. Her parents came from the western end of France very close to Switzerland. You hear about French being Catholics and drinking wine. These were Baptists and they had no wine. I have an idea that maybe they left France because of persecution by the Catholics. In any event, that's her history. She took up piano on the farm. They had a melodeon that you pumped. The thing actually had a keyboard, and it looked like a piano. She actually was a pretty good pianist.

A. H. Norton got this call to come to Cook Academy, which he accepted. It was a coeducational boarding school. His job was to fill up those rooms. Floor two was female, floors three and four were male. One day he got a communication from the Chinese

ambassador. (I don't think I have his name accurately, so I'm not going to mention it.) He was told to get the best academy in the whole United States because they were going to be the first four boys ever to come here from China to get the education necessary to go to college here. The Chinese ambassador didn't know anything about American high schools. He did his high school work in China. But he knew a sophomore at Cornell, named Alfred Sze. So he wrote him. He didn't know anything about them either, but he had an American roommate, and he asked him where the best one was. Well, he named Cook Academy where he came from, only twenty miles west of Ithaca. Thus was the best high school chosen. Father learned afterwards that if he had turned it down they were going to close the school. [laughter]

[the following addendum is excerpted from 1 March 1991 letter of C. D. Hurd to J. J. Bohning] One of the four Chinese boys who arrived at Cook as juniors in 1904 was V. K. Wellington Koo, age sixteen. Until recently I was unaware of his background that led to his answer when Father (A. H. Norton, later referred to as AHN) asked him what he was planning for in life. "I plan to be a statesman." So, a program in that direction was arranged. Four years ago the editor of the New Yorker wrote up an interview with Koo (April 18, 1987 issue). As a boy in Shanghai he passed by the British Commission, which carried the sign, "Dogs and Chinese not admitted." His thought was that it was bad enough in any event, but to put dogs ahead of Chinese was too much! Hence, he started on a career of diplomacy, starting at Cook Academy.

Well, Koo did four years' work in three and graduated as salutatorian of the class. He then went to Columbia University, majored in political science, and graduated in three years. During these years, however, he also was the leader of Columbia's debate team, and was editor of the Spectator, the college daily paper. Continuing at Columbia in study towards the Ph.D. degree (he was to get it in June three years later), but in March of that year President Yuan Shih-k'ai of China, who followed Sun Yat-sen who was the first president, cabled him to come and become his English-speaking secretary. His adviser was John Bassett Moore, who had been Assistant Secretary of State under Presidents Cleveland and Harrison. He urged him to accept, for his doctoral work was essentially completed. He received the degree in absentia in June.

At age twenty-nine, only thirteen years after he told AHN of his intention to be a statesman, he was named ambassador to Washington. Some time later, he became ambassador to London, and then to Paris. He was Chinese delegate to both the League of Nations and the United Nations. In 1926-1927 he was president of China.

Now, back to Cook Academy. In the early 1900s the Chinese in the U.S. were on a much lower social stratum (as laundrymen) and usually not highly regarded socially. A committee of students at

Cook did not want them there and imagined that AHN did not want them there and that they could help him, but how wrong they were. They came to Father's office with the ultimatum, "Either they go or else we will." He replied, "I had hoped that all of you boys would stay but if you feel you must leave, I'll be sorry. The Chinese boys, however, will stay." As you can guess, they all stayed. By 1910, the two best known U.S. academies in China were Phillips Andover and Cook. The Chinese at Cook (six to eight per year) evidently sent back good reports.

I was nine years younger than Koo, but we often took hikes together. He also taught me how to use a jackknife that I had received as a gift. [end of letter]

BOHNING: You went to Cook Academy, but did you have any schooling before you went there?

HURD: Well, I had grades one and two in Mexico. I had grades three to seven in the Montour Falls grade school. I had grade eight in Cook Academy because Father thought it would simply be better that way. I had my first two high school years in Cook Academy. So I was at Cook, really, for three years, plus that was our home.

He had a call from Cook Academy to become professor of mathematics and astronomy at Elmira College, which is now coed, but at that time was altogether female. He was there about two years and the president appointed him vice president of Elmira. Afterwards, while he was at Elmira, there was World War I, and he was on a leave of absence while he was the vice president to go over to France and hand out coffee and donuts to the boys in a YMCA hut that he was head of which was very close to the front lines. While he was there he got a letter asking him if he would consider being president of Keuka College, that was going to be newly reopened under entirely different auspices. Keuka annoys me right now because they advertise "founded in 1890". What was founded in 1890 was Keuka Institute, the high school. The Reverend George Ball very badly wanted to have a college, but he had simply run into debt every year, and his three nephews bailed him out. They were the Balls of Muncie, Indiana that make fruit jars. So they had some cash with which to do it. He had a college that started about 1896. That was the first of the Keuka Colleges. It actually lasted till about 1912. I don't think they had more than forty graduates. They'd take two years and go to Syracuse or Cornell to get a degree from a respectable institution. [laughter]

BADER: It still exists now.

HURD: Yes, but not that one. It is the very same original building that's there. It's a big building. But the present Keuka College is one that Father opened after the other one had actually been dead for about four years. The earlier one was coeducational. The one that opened was female, like he had at Elmira. He said that's the only way he would actually take it, and he thought he could make that go. It was sort of started by the New York State Baptists, who put up part of the money. But he had already gotten a pretty big amount. That was the year that the Baptists persuaded him to put what he had raised into the whole total that they had, and they would of course get it back plus more. What he got back was about two-thirds as much as he put in. It was the worst thing that happened to him. He was scrounging for awhile just to keep alive. You have to be real careful about the Baptists. [laughter] In any event, he did come back, and his first daughter, my older half-sister, was the first one that registered. He probably proclaimed that. Now, in starting that particular school, he didn't want any antagonism with the old Keuka people. As a matter of fact, he wanted them to give all the help that they could, and many of them were in that Keuka Park area. Keuka Park is a town four miles south of Penn Yan. Penn Yan is at the north end of Keuka Lake. Keuka is the Indian word for crooked. That's the only one of the finger lakes that's crooked; it's shaped like a Y. All the rest are straight up and down.

BOHNING: I would like to ask you a little bit about your early education and some of your experiences.

HURD: I've already told you that I had my grade school at Mexico and finished at Montour Falls, the last year of which was in Cook Academy, and then I went to Elmira Free Academy, in the city of Elmira; that's where Father moved to. But I took a post-graduate year in high school because they didn't think I was heavy enough to go away to college. So I was in high school, really, for five years. I found myself pretty well prepared for college.

BOHNING: What kind of science-related experiences did you have in high school?

HURD: In Elmira I had chemistry and physics and mathematics, all under very good teachers. I think the physics teacher was the best one I ever had. There was a good chemistry teacher in college, too, but in high school the physics teacher (Dillon Cady) was certainly the best. Chemistry wasn't bad either. I had John W. Kern in chemistry. Afterwards he got a job teaching at Mercer University in Georgia. He didn't know what I was doing, but at that particular time I was working on a Ph.D. program at Minnesota, which I completed at Princeton. He wrote me by way of my parents that he'd like to have me come down and

help him teach and he would take a year off and start work on his Ph.D. at Illinois. After he had that, I could also, if I wanted, go to graduate school. Well, I had to reply to him that I was already in graduate school. The first job that I had was at Urbana after graduating from Princeton. Kern was there as a graduate student, and I was teaching. [laughter]

BOHNING: A reversal of roles.

HURD: Yes.

BOHNING: I'd like to talk a little bit more about your first chemistry. Was that your first exposure to science or had you had anything previous to that?

HURD: I'd had so called grade school science, which didn't amount to much. But I at least recall sitting in the classroom where I had it.

BOHNING: When you took the high school chemistry course, did you feel at that point that chemistry, physics or math or science area was what you were really interested in? I'm sure you had the classics and literature and history as well.

HURD: No, I liked them all. I hadn't really made a choice as to which one I would take because of the high school courses I took.

BOHNING: Why did you select Syracuse for your undergraduate work?

HURD: Well, Father went there and I would imagine he kind of thought it was a pretty good place for me to go. After all, it has to be the best one, you know. [laughter] I ought to have actually gone to Western Ontario. [to Bader] Isn't that where you went?

ISABEL BADER: Queen's [University in Kingston, Ontario].

BADER: Western Ontario in London [Ontario]?

HURD: Yes.

BADER: It's not a bad school.

BOHNING: When you went to Syracuse, did you select chemistry as a major initially?

HURD: No, I took it because one had to take a science, but I took both chemistry and physics. I took geography in my graduate year in high school, as a matter of fact. Then I took some geology on top of that for one year at Syracuse. I also took a course in astronomy.

BOHNING: When did you make the decision to become a chemistry major?

HURD: When I had a course in organic chemistry under Reginald S. Boehner, trained in Canada. He is probably the best teacher I ever had.

BOHNING: What was the chemistry department like at Syracuse when you were there?

HURD: It was pretty good from a teaching point of view. It was not very good from a research point of view. I hope they're getting better, but at that time there was one person who thought he was doing something. As a teaching school, though, they had good teachers. I had H. C. [Hermon Charles] Cooper in physical chemistry. I had Merritt Leroy Grose in analytical chemistry. As a matter of fact, he's a brother of Professor Grose of the history department here, who just recently died. I think that our history professor was a better teacher than his brother at Syracuse. I'm not putting down that he was awful, but he certainly was nothing like Professor Boehner.

BADER: Syracuse now has really two different schools very close to each other. Was that the same then? You know, you had the University of Syracuse, and then you had the State University of New York.

HURD: That's the same thing.

BADER: They're separate now.

HURD: They have the same adjoining campus. I haven't kept up with the legal aspects of it. They've got a pretty big campus. I think they've got one of the best forestry departments in the world.

BADER: Michael Szwarc was there.

BOHNING: Yes, I did an interview with him (1).

BADER: With Michael Szwarc? He's a fun guy.

BOHNING: Yes. [to Hurd] What kind of laboratory work did you do, especially in organic chemistry?

HURD: Well, the organic course was elementary organic. You did the laboratory that was part of that course.

BOHNING: Was it mostly synthesis?

HURD: Yes, or separations or purifications. It's probably taught about the same way yet.

BOHNING: Do you remember any textbooks that you may have used?

HURD: Yes, there was a book by [A. F.] Holleman and Cooper that was used in the inorganic chemistry course that I took there (2). (That was the Cooper that was a professor there.) He didn't write the book; he translated it from the book that Holleman published over there in the Netherlands, I think, but I'm not sure. He also put out an organic book, but I didn't ever have that one (3).

BOHNING: What was it about organic that interested you?

HURD: Perhaps the way it was taught. But it appealed to me because it overlapped so much of what we actually all do.

BOHNING: Did you do any kind of research as a senior, in your last year at Syracuse?

HURD: No. That began at Minnesota. However, World War I was on. The day after I actually graduated from Syracuse I was taking the train to Washington because what they had was a poison gas problem. That was part of the infantry, so they actually started a Chemical Service Section. That's what it was called, the CSS. What we have now is Chemical Warfare Service, but that was a development out of the CSS. And they had a research division. They had trouble filling it up because the chemists that were already in jobs in teaching or industry didn't want to throw those away and then discover that when the war was over they wouldn't have a job. So they kind of leaned very heavily on the graduating seniors.

I was happy to do it, because that actually was a job. I arrived there, and it took me about half an hour to get from where I was at the end of the line up to the desk where I could become a buck private. In that half hour, I got pretty well acquainted with the person right behind me. He was doing the same thing and came from the University of Georgia. His name was Alfred Scott. We were told that they had no army camp for us there, but we had commutation, which means money from which we could buy our own room and board. Alfred and I had talked it over and found we were pretty compatible with each other. We said, "How about renting a room together?" That would save money, which it did. So we got a room there in Chevy Chase, Maryland, which was pretty close to where we had to work. I went to American University, and he went to Catholic University, where they had a few rooms open, but not many. American University had a campus already built that hadn't been used yet as a school. So the army just took it over. I was under M. F. [Melville Fuller] Coolbaugh and Gerald Wendt at that time. They were pretty prominent chemists. Coolbaugh came from Colorado. There's a Coolbaugh Hall there that's named after him. Wendt at the time, was at the University of Chicago in physical chemistry, and I was under him. I was in the inorganic division of the research section. What do you know about that?

BOHNING: What kind of work were you doing?

HURD: I was making some chemicals that were for others to use in tests as war gases and purifying some that weren't pure enough.

BOHNING: This was in 1918?

HURD: 1918. For half a year. I thought I was pretty good there at the time that I began. My god, that was the best bunch of chemists in the United States that were there; J. B. [James Bryant] Conant and Roger Adams, for example. I was under L. W. [Lauder William] Jones, who was not really in the army at all. He was a civilian that was put in charge of offensive research.

Offensive research is poison gas. Arthur B. Lamb was put in defensive research. That means gas masks. I was in the offensive part so fundamentally I was under him but he was way up and I didn't see him. Half a year afterwards at the armistice, Alfred Scott and I both decided that we would go on for graduate work. We hunted him up and asked him if he would take us. I think we must have been the first two that asked him. He said he'd be glad to, if we paid our own way. [laughter]

[END OF TAPE, SIDE 1]

HURD: I had to pay my own way for only a few months before I got a teaching assistantship. Then that changed to a du Pont Fellow, which put me on full-time research. I was there two years under Jones. I had him both in the army and at the University of Minnesota, where he was the dean. He didn't enter the army, but did his work there as a civilian. In two years, though, he got an offer to transfer to Princeton. So he took me along to Princeton as a graduate student, and also Alfred Scott. We continued to room together at Princeton.

It was about two years after I began to know Alfred Scott that I discovered he had had some additional experience. At Minnesota he'd say, "Charles, what do you say we walk over to the gym and shoot a few baskets?" Well, he could shoot a few baskets. I could hardly pick the ball up off the floor. One day we were there and the coach of the team was there, named Doc Cook. He was an M.D. who didn't practice. He was just the basketball coach. He came over to me and said, "Who's that boy?" I said, "He's a graduate student in chemistry." That meant he couldn't use him. I discovered afterwards that Scott was the best player the South had ever produced and he's in the basketball hall of fame.

BADER: Really? Why couldn't he use him?

HURD: Because he was not an undergraduate.

BADER: Oh, I see.

HURD: They could certainly have used him as an undergraduate. Actually, that was the year that Minnesota had the Big Ten championship of ten victories and no defeats. At the end of the season four of them, plus Alfred toured Minnesota playing amateur basketball, for which they might have a handkerchief that would sell for $25. [laughter] But he was the best one in that group of five. So he was pretty good. He went back to become the chairman of the chemistry department at the University of

Georgia, which was in a pretty sad state until the older ones died off. He's the one who really is responsible for it having become a pretty good school in chemistry.

BADER: This is in Athens?

HURD: Yes, in Athens. That was his home. He died a few years ago, I think of smoking.

BOHNING: In the summer of 1917, while you were at Syracuse, you did some work with Thomas Edison. How did that come about?

HURD: Father thought that in the summer vacation that was coming up I should do something because of World War I. He knew that Mr. Edison was the chairman of the Naval Consulting Board. So he simply wrote him and asked him if he had a place for me for the three months of the summer. Perhaps I could help the military in some way or another. His secretary, William H. Meadowcroft, wrote back and said, "Yes, send him on. We'll pay him fifteen dollars a week." Not a day or an hour, a week! I was put in a team of four or five that would take an automobile up the Orange Mountains towards Morristown but not quite to Morristown. That was an open hay field they were able to get. They had the problem of where that shotgun or where that cannon or where that stick of dynamite was coming from. They actually went at it by triangulation. There was a base line with three of those Ediphones on it. Microphones transmitted that sound to three pins that would make an indentation in a revolving wax record. Then under a microscope, the difference between the three indentations made by the three pins could be translated into distances, such as yards or miles. We ultimately got pretty good. We would take temperatures and wind velocities in that area. In other words, the speed of sound isn't anything at all comparable to the speed of light, it's pretty slow relatively and you had to make corrections; otherwise it wouldn't work.

BOHNING: Did you meet Edison at all?

HURD: Oh, yes, he was there. We would ordinarily meet in a room and come back to that room, and he would ordinarily be there and talk to the person that headed it up. He was hard of hearing at that time, and had a high-pitched voice, maybe for that reason. He could overhear much of what they were talking about. But his favorite word was "rotten" [said by Hurd as an imitation of Edison]. [laughter] I have an idea he didn't think we were doing too well. [laughter] I was only there for the three months of the vacation and then went back to complete college.

That was in West Orange. There was a Baptist church in Orange but it was called the North Orange Baptist Church. There's an East and a South and a West Orange, but no North Orange, except for that church. There, I met the lady that ultimately became my wife, four years before I got the Ph.D.

BOHNING: Yes, you were married in 1921.

HURD: August 29. How's that?

BOHNING: Yes. [laughter] So you corresponded with her, because she lived in New Jersey.

HURD: Yes, she lived in New Jersey. She was with a hat company. Orange at that time was quite a hat center.

BOHNING: But you came back to Princeton in a few years, so you were closer.

HURD: I was only at Princeton one year, but that was considerably closer than Minnesota. Once in a while we would meet, and it was during that period that I proposed to her, if she was willing to wait.

BADER: Your stepfather took very good care of you, I take it. Your stepfather was very close to you.

HURD: He didn't have much money, but he at least was sure that somehow or other I was going to school. At that time New York State had a state scholarship system; the highest five students in any scholarship district would get a scholarship of a hundred dollars a year. I happened to be in that category. The districts were, in essence, counties. In other words, my home was in Schuyler county. In the metropolitan areas, though, it was not. They had it broken up into New York City, Brooklyn, Syracuse, Rochester, into several scholarship districts. But I was in a rather poorly populated county. The biggest town was Elmira, and all the rest of them were quite little, like Montour Falls. But that helped, and Father said he didn't know how he could have arranged it, if I had not gotten that hundred dollars a year for all four years. At least, I had to be aware of it a few years ago, and at that time it was one hundred and fifty dollars. I don't know what it is now, probably two or three hundred dollars, but it was a tremendous help to our family. In

the summertime I also sold Fuller Brushes to help out. That was up between Utica and Ogdensburg. I took the trip on foot, and really made quite a lot of money.

BOHNING: There was a lot of door to door selling going on in those days.

HURD: Evidently. The other one that was big was aluminum pots and pans. It was quite new and it hadn't reached the stores yet, so people were punching doorbells.

BOHNING: When you were at Minnesota with Jones, did you take any courses?

HURD: Yes. That was my first year of graduate school. I took the freshman chemistry over again. My high school teacher Dr. Kern thought I was foolish to do it, but I took freshman physics over and I took freshman chemistry over. The first math I took at Syracuse was analytical geometry. That was under Dr. [George F.] Metzler. There were two Metzlers in the math department. I had the poor one. His brother, [William Henry] Metzler, was the head of the department. I asked him a question once that was unrelated to the analytical department, and he said, "I'll ask some other people in the department about this." Of course, I knew it was his brother he was going to ask.

BOHNING: I have a list of some of the faculty at Minnesota.

HURD: I had a course under M. Cannon Sneed. William H. Hunter taught the first organic course I took, then I had another one under Jones.

BOHNING: When you started your research with Jones, did he assign the problem to you?

HURD: No. With him, I first had a course of organic preps; that was not research. After that I hadn't picked out anybody yet; I was just under him because that was the course he was teaching. Then I asked him if he would take me and he seemed happy to do so.

BOHNING: What kind of person was he?

HURD: I'd say wonderful. He was easy going in a way, but if he said something was going to be done, it was done. He was very popular.

BADER: When did [Izaak M.] Kolthoff come?

HURD: Kolthoff was there also in analytical chemistry.

BADER: Did you get to know him well?

HURD: No. You see, I didn't have any analytical chemistry there. I knew who he was, but he probably had no idea who I was.

BADER: Kolthoff is still there.

HURD: Another analytical chemist was L. W. McCay of Princeton. If you were a graduate student, you had to prove that you knew some analytical chemistry. If they were from Princeton, as many of them were, he knew that they were pretty good so they didn't have to do anything. But coming as a transfer from University of Minnesota, I had to prove it. He handed me a mineral and told me to analyze it. I told him it was pretty much antimony, but with a trace of arsenic in it. It was stibnite. He said, "Well, you can analyze that for arsenic." [laughter] "Inasmuch as you are an organic chemist, though, I'll tell you that you can analyze it by the Emil Fischer method. [laughter] If you don't know what the Fischer method is, you can look it up, the same as I had to (4). [laughter] But in essence, it involved converting to the chloride, followed by distillation. But it took a month out of the last year of my graduate program.

I was only at Princeton for half a year according to Dean Andrew Flamingo West, "forty-nine inches around the vest." [laughter] Because I was a teaching assistant at Princeton, on his books I was only there half-time. But Jones had made arrangements with the faculty that if he was going to come, there wasn't going to be any handicap to prevent me from graduating at the same time I would have if I had stayed at Minnesota. It's a good thing he insisted on that, because I was only there a half a year and West dearly wanted to have me there officially a whole year at least so that I'd know there was something at Princeton. That would have required my being there two years.

BADER: Did you get the Ph.D. from Minnesota or from Princeton?

HURD: From Princeton, under the same man, L. W. Jones.

BOHNING: How much of your laboratory work had you done at Minnesota?

HURD: A pretty heavy amount of it.

BOHNING: I was interested in the paper which came out of your Ph.D. thesis (5). There are two things. One, given your interest in nomenclature, many, many years later, you were actually struggling quite early on with naming things in that very first paper with Jones.

HURD: Yes.

BADER: You still are now. [laughter]

BOHNING: But that's what struck me. Back in 1921, when that paper was published, you were already suggesting prefixes and trying to sort out the nomenclatures of some of those acids. The other point was that you were already looking at what later became some important mechanistic organic chemistry. You were relating the rearrangement of the stability of the radical, and that was pretty new at that time to start thinking in those terms, wasn't it?

HURD: Actually, it will interest you to know what happened there. As you know, Moses Gomberg had isolated the triphenyl methyl radical, so Jones was after me to make triphenylacetohydroxamic acid, which would rearrange with the triphenylmethyl group. Would it be very much easier, or wouldn't it? Well, it was a trifle easier, but not awfully much so. But in that particular process I also made diphenylacetohydroxamic acid from ethyl diphenylacetate and hydroxylamine. That was simply a repetition--the compound was new, but the method was not. It was simply an ester plus hydroxylamine to give the hydroxamic acid. But to do something new, I decided to make it from diphenylketene, which had been made only half a dozen years earlier. That worked excellently as well, but that put me on to what I was planning to do at University of Illinois when I got there, because ketene itself was comparatively new. I said, "I'm going to see if I can get ketene and have it act with hydroxylamine."

Well, it took me quite awhile to get the ketene. [Julius] Schmidlin and [Maximilian] Bergman had produced it by heating acetone to about 650 degrees (6), which is just on the borderline of whether it will decompose or not. It took me pretty close to half a year before I could actually get it. What we had was a tube and combustion furnace. You passed the acetone vapors over

some broken up chips in the tube and then you get ketene coming out of the other end which you collect by passing it into water or alcohol or ammonia or things like that. But that got me into the chemistry of ketenes, which I would not have done at all if it hadn't been for the dissertation work that involved a ketene. Also, the trouble that I had in heating that hot enough got me into high temperature decomposition reactions in two ways. One was experimentally, and then I decided to author a book because I discovered there wasn't any such book (7). A friend of mine at Harvard, namely Conant, was doing it. He wrote and said, "I've got a name for it--pyrolysis." So he's the one that really bit.

BOHNING: Just a little. [laughter]

As you were finishing up your work at Princeton, had you thought about an industrial career versus an academic career?

HURD: Yes. As a matter of fact, I thought that probably it would have to be in industry. But then W. A. [William Albert] Noyes wrote to Princeton asking if they had anyone whom they would recommend. I think two of us went there.

BOHNING: Who was the other one?

HURD: Harvey Neville.

BOHNING: Oh, yes. Isn't the chemistry building at Lehigh University named for him?

HURD: Yes, that's where he transferred to. Illinois hired about six people that year. One was in the problem of sewage and things like that. The chemistry department handled that. Two of us were in organic chemistry, and one was in physical chemistry. That was [Thomas E.] Phipps. The other one in organic chemistry was a woman, Edith Nason. I haven't heard much of her. She transferred to Vassar and then went to Syracuse, I believe.

BOHNING: What was Illinois like when you got there? What were you assigned to do?

HURD: I was teaching freshman chemistry with B. Smith Hopkins for two years. Then I transferred into organic chemistry, where I worked more with [Carl S.] Marvel and [Roger] Adams. As a matter of fact, I had Adams' office as my office for one year

because that year he was in Europe. I also had the laboratory next to him, and the only other person in the laboratory was Wallace Carothers.

BOHNING: Really?

HURD: Yes. We got to know who we were if we passed each other on the street. But you know, he was kind of a funny fellow. He didn't want to talk much while he was working. [laughter]

BOHNING: Is there anything else with Carothers or Adams or Marvel? What kind of interactions did you have with them?

HURD: Fundamentally I was doing my own research. I knew who they had under them but I had nothing to do with what they were doing.

BOHNING: Did you talk to them at all about your research and what you were doing?

HURD: No. But they actually knew what I was doing because they had a plan of having the people talk to the faculty on what they're doing. One day it was my turn and afterwards Adams came up and told me that he thought I was doing pretty well. [laughter]

BADER: At that time did you have a laboratory making chemicals for Eastman Kodak?

HURD: No. Marvel and Adams were involved there, but I was not. That was sort of a way in which they paid the graduate students, particularly in the summertime. They'd stay there and work full time for full pay.

BADER: That's why the deliveries of Kodak in September were much better than in March. [laughter]

[END OF TAPE, SIDE 2]

BOHNING: You published at least seven papers from that work at Illinois, mostly dealing with pyrolysis and ketenes (8). You really developed the best way of making ketene compared to what had been in the literature up to that point (9).

HURD: Well, it was an eleven percent yield according to the work of Schmidlin and Bergman (6). After a half year I was able to get that up to about seventeen or eighteen percent by heating it to 700 instead of 650. But heating it that hot was not conventional in those days. That is, the combustion furnace would hardly do it. So I got some help from the chemical engineering department making a metal tube, not for analytical purposes, but for passing things through it that were pretty hot. That worked. That was the way that we got it up to about seventeen percent. Then one of my students decided to have a ketene lamp, and that put it up to about nineteen percent yield, of the part that decomposed. We had a Chromel wire that was heated to glowing, and then underneath was simply a bucket of acetone that was boiling. The vapor passed up through that on the way out, and then you'd pass the ketene into what you wanted it to react with. I think that is the only way to make ketene yet because it gives a good yield.

BADER: Did you do much work with diketene also?

HURD: Yes.

BADER: I remember when I sent in a paper to Marshall Gates showing that diketene forms an adduct with acetone (10), he wrote back to me saying, "It's hard to believe because after all the work of Hurd and the work of Union Carbide, if it really did form an adduct with acetone, they would know about it."

HURD: Well, we didn't. But you did.

BADER: It was a bit of luck. In all those years, of course, there was a doubt where the double bond in diketene was. The problem was solved by NMR and Herb Gutowsky. (32)

HURD: Well, you see, he was an old ketene man, too. [laughter] Perhaps you're an acetone man. [laughter]

BOHNING: You mentioned the graduate students that you had at Illinois. I counted at least five, and you were there only three years. I wanted to ask you how it was attracting graduate students, given that Adams and Marvel were there. In three years, you had five graduate students.

HURD: Not necessarily. I had perhaps that many students who worked with me, but most of them were seniors.

BOHNING: I see.

HURD: In other words, my first one was Paul Cochran. I've kept in touch with him ever since. We exchange Christmas cards yet. He actually is the one that got the ketene thing moving. Then I had a person that lived in Cicero. His name was [Cyril] Kocour. He headed up the Kocour Chemical Company. Then I had a person that was a senior but he became a graduate student. He actually transferred up here with me.

BADER: At that point Illinois was the best school in the country.

HURD: Yes, it certainly was a big one and a good one. Northwestern was quite young. That is, the institution is old, starting in the 1850s, but the chemistry department began in 1920 with W. Lee Lewis, who was part of the army. He was at Northwestern as an associate professor when World War I started, and he was transferred to the army at the Catholic University of America. He was in offensive research, and the problem that was assigned to him by L. W. Jones was to prepare some new vesicant that had arsenic in the molecule. "That will probably make it more poisonous." He wasn't getting anywhere at all.

One day he was talking about it to the Catholic priest that taught chemistry at Catholic University. He said, "You know, I remember about six or eight years ago we had a boy here that was earning his Ph.D. by studying acetylene. One of the things he passed it into was arsenic trichloride and he got the most god-awful mess. He decided not to do anything at all to purify it. He just wrote up a page in his dissertation and went about doing some more pleasant things. That was just what Lewis wanted to know, so that afternoon, he got a tank of acetylene and passed it through arsenic trichloride. Nothing happened. It just bubbled in and bubbled out. He went back to the priest and told him that. "Oh, yes, I remember. He also had some anhydrous aluminum chloride there." [laughter] And thus was the birth of "Lewisite." That person with acetylene was Father [Julius A.] Nieuwland, who earned his Ph.D. there and then went to Notre Dame, where he spent the rest of his life on acetylene. He was a charming man, too. Did you ever know him?

BOHNING: No.

HURD: Well, he was, really, I think, one of the best. [to Bader] You probably knew him, didn't you?

BADER: Well, I've heard him. He didn't know me. I knew him.

BOHNING: Did you have interactions with him?

HURD: With Nieuwland? Only as an organic chemist. That is, he was at Notre Dame and I was here. But he had me down once to give a chemical talk.

BOHNING: When you went to Illinois, what was your position? Were you an assistant professor?

HURD: I was an instructor, and I stayed that all three years that I was there. I moved up here to become an assistant professor. If I had stayed there, Marvel told me that I'd be promoted probably to an associate. They had that rank there then, although I don't know if they do yet. It's a rank we don't have. It's between instructor and assistant professor.

BOHNING: How did that move up here to Northwestern come about?

HURD: Well, by hiring a van.

BOHNING: No, I meant... [laughter]

BADER: How did they twist your arm?

BOHNING: (You're keeping me on my toes here.) Were you looking to get out of Illinois?

HURD: No. As a matter of fact, I was even thinking about not accepting it. But what had happened was, at the University of Minnesota, L. W. Jones had brought up Frank Whitmore for his teaching there at Rice Institute. He had earned his Ph.D. at Harvard and then he went to Rice for a year and Jones got him the next year as an assistant professor. He transferred here at the request of W. Lee Lewis. The previous head of the department here was [Abram VanEps] Young, who hadn't done anything. He resigned [in 1918] and died about two years later. So Lewis had to have a replacement for Professor Young. (Young didn't have a doctor's degree. He sort of shopped around at three or four universities in Europe, but never actually stayed anywhere to get a degree.) But that made the opening for bringing Whitmore here.

I think Lewis and Whitmore applied to the Public Health Institute in Washington to get funds to do some work on arsenic as a medicinal. Lewis had recalled that ten years earlier [Paul] Erlich had published on Salvarsan (arsphenamine, "606") as the

best drug for curing gonorrhea and syphilis. They were able to get that. Whitmore asked for it because of his work on mercury; Lewis asked for it because of his work on Lewisite. That put some cash in the hands of the department with which they could attract some graduate students. That's the way they got it.

In 1924 Lewis decided that he would resign to become the director of the Institute of American Meat Packers down at the stockyards in Chicago. That left an opening. But you see, Whitmore had known me at Minnesota, because he was there at the same time I was there as a graduate student. Probably that's the reason he went down to Illinois to see if I could come up here for a vacancy to take the place of Lewis. Not the title, just to teach organic. Whitmore taught mainly freshman chemistry.

BADER: But the decision must have been difficult, because Illinois was such a superb school. Northwestern is a very good school today, but it wasn't then.

HURD: It wasn't then, that's right. They had Lewis and Whitmore in organic chemistry. But Lewis left, so I had the organic chemistry. They had Ward Evans--a charming personality, but not really a great physical chemist. But he was a raconteur par excellence. [laughter] No, the institution was kind of weak in research.

BADER: Well, you made an enormous difference.

HURD: Whitmore did, really. He was the dynamic person who got things going here. After I came here we both applied for the American Petroleum Institute grant. But we were both able to have it. He worked on his Whitmore type of rearrangements. Actually, that was entirely based on his experience with Jones. Whitmore's chemical theory taught at Harvard was nil. He'd put a proton onto an alcohol and get an olefin out of it, and if he'd take the right one where there wasn't any hydrogen next door, the methyl group wandered.

I was at an ACS meeting where he actually gave that as a talk. At the close of the meeting, he was asked by somebody in the audience, "You haven't used the word carbonium ion there, but aren't those carbonium ions that create the move?" "Oh, no, no, those aren't carbonium ions." He knew that Jones had a hydroxamic acid in which a radical moved. But Jones had taken an anion and lost an anion, which means there was no actual charge that was created or lost. But in Whitmore's case, he put a proton on a neutral compound, and he lost a neutral compound, so he had to have a plus charge. But he didn't know that. He just

rationalized from Jones's type of rearrangement to his that they were both neutral. [laughter] He hadn't learned how to count electrons. Well, he learned pretty soon.

BADER: How long did he stay at Northwestern?

HURD: For about ten years. The last two years he was here, he was primarily in Washington, D. C. as head of the National Research Council division of chemistry. So he actually appointed a committee of three of us--[Murray A.] Hines, Evans and Hurd. He hadn't thought too much of Evans, and Hines hadn't published anything, so he had me be sort of an executive secretary on matters that had to be treated. We handled everything except the pocketbook. He came up here when he had to talk to the dean about money.

BOHNING: What did he do to convince you to move from Illinois? What was his selling point?

HURD: As I recall, I think he wrote a letter. Probably it was the higher rank and higher pay that persuaded me. I actually started out at Illinois at $1800 a year. That would be chicken feed today in hiring anybody.

BOHNING: What did Whitmore give you here, when you started at Northwestern?

HURD: I think I got $2400.

BADER: That's a nice percentage increase.

HURD: Yes.

BOHNING: When you came here, then, were you teaching primarily organic?

HURD: Yes, that's correct. I had the elementary course and usually also the advanced course.

BOHNING: What was Whitmore like to work for?

HURD: He was a hard worker and he expected hard work of other people. From that point of view, he was easy to get along with. I think if you hadn't been an equally hard worker, he would have pushed you along into it, or at least tried to. We had some persons that were not, really. But they were older than he was.

BOHNING: You just commented about Whitmore's not being able to count electrons at the time that you were starting to teach organic. The whole Lewis concept was still pretty new at that point.

HURD: You mean G. N. Lewis, not W. Lee Lewis. [laughter]

BOHNING: Yes. I'm sorry. G. N. Lewis.

HURD: That was brand new. Terms like "leaving group" hadn't been coined yet. The octet theory of G. N. Lewis was really brand new.

BOHNING: Did you introduce that in your teaching?

HURD: Yes.

BOHNING: Do you remember what text you used when you first started up here?

HURD: I think Norris (11). I had Perkin and Kipping in college (12). You probably haven't heard of it, but it was a British text, and a good one.

BOHNING: Is that the Kipping who discovered silicones?

HURD: I think so. I think I used the text by Norris. The terminology was really terrible. I recall once at an ACS meeting when F. B. [Frank] Dains was chairman. (He was a grand old fellow from Kansas.) At the end of the business meeting, he said, "Are there any other comments?" There weren't any at all. After half a minute, I said, "How would it be if we had a committee to study the improvement of nomenclature?" Dains was very sympathetic to that. Probably he'd had the same trouble with textbooks that needed some improvement. He passed that idea on to Whitmore, who was then the chairman of the organic

division. Well, Whitmore passed it on to me. [laughter] So I became the chairman of a committee of one to do something about nomenclature.

BOHNING: That committee existed for a long time, didn't it?

HURD: It certainly did.

BOHNING: How sympathetic were other organic chemists to your attempts to sort out nomenclature?

HURD: Well, I'll tell you. I started out by taking a lot of texts and journals, sort of thumbing the pages for words that were used for certain structures. If they deviated from what I thought they ought to be, I put them in a list. I had a list of about one hundred names that I mailed to approximately one hundred organic chemists who I thought well of. I asked them if they had any comments, good or bad. I was amazed at the almost uniformity of the fact that they said this is the type of thing that we ought to do. So to answer your question, I kind of had them be a party to it. I found out what they thought, and they all thought it was pretty well. I think most of them were pleased they didn't have to be on a committee. [laughter]

BOHNING: Did any organic textbooks change as a result of this? Did you make an influence on authors?

HURD: I didn't study it from that point of view. I would study a text reasonably carefully if I thought I was going to adopt it. But after I was up here for a period, I didn't keep on teaching the elementary course too much.

BADER: I still have Aldrich Catalog Number 3, corrected by you.

HURD: Number 3?

BADER: Number 3. Numbers 1 and 2 were just one page documents.

BOHNING: [to Bader] You had indicated to me earlier that Dr. Hurd was still correcting the nomenclature in the Aldrich catalog.

BADER: It was just a month ago that he sent me something about isonitrile.

HURD: I think you [Bader] have done more for good nomenclature than you possibly think you have.

BADER: We had to because of you.

HURD: It shows in your catalog and your good use of names in the bulletins that you publish.

BADER: You don't know how often I've said to our people, "Boy, what Charles Hurd would say to that! Change it!" [laughter]

HURD: Eastman Kodak isn't that careful.

BADER: If I had $20 billion in camera sales we wouldn't be either. [laughter] It was Union Carbide that had lots of bad names.

HURD: Certainly was. That's right.

BOHNING: Why is nomenclature such a problem? I taught some elementary organic to nursing students, and of course you deal with basic nomenclature in that course.

HURD: That was the first course I taught here. I came up here in the summer session. I had a bunch of hospital nurses. They worked all night. I had them at eight o'clock in the morning and they were half asleep. [laughter] I had never taught such a course in my life. Did you also have that experience?

BOHNING: Yes. It's an interesting group of people to deal with, that's for sure. But there's an interesting relationship there with nomenclature, because many of those students don't see a need for studying chemistry. And yet nomenclature, in terms of the drugs and everything else they have to deal with, I think is very important. I always tried to impress upon them this idea of how much you can get out of a name, by looking at a name and understanding something about the compound's structure and also what it might do by the nomenclature. To this day, in a hospital, if somebody wants to give me something, I make sure I read the label too.

ISABEL BADER: They make mistakes.

BOHNING: They make mistakes. That was one of the things I was trying to stress to them was this importance of nomenclature.

HURD: Well, is the word "aspirin" part of nomenclature, or would it have to be the actual name that's also on the bottle?

BOHNING: Yes. I agree that "aspirin" is not nomenclature in that sense, but this exchange you just had about people publishing things that are careless about nomenclature is very interesting.

HURD: Yes.

BOHNING: That was one of the fun parts of organic chemistry long ago when I was a student, how important it is to be able to convey something through the right name. When you have companies who aren't doing that, paying attention to what they're publishing, I would think it can create difficulties. That's why I said you were struggling with nomenclature back in 1922 in your Ph.D. thesis (5).

HURD: I had no help, so I had to do something. [laughter]

BADER: Did you travel to Europe at all in those days?

HURD: I have never gone to Europe. What's over there? [laughter]

BADER: As you know, a good many American chemists did go over to study in Germany or England.

HURD: I didn't have the money to do that. I stayed home.

BOHNING: I'm not clear on the dates between assistant and associate professor. You started here as an assistant professor in 1924. When did you get promoted to associate?

HURD: I think 1928. I got to be a full professor about 1932 or 1933. Then about 1948 or 1949 I became Morrison professor. I had the title given in place of an increase in pay. [laughter]

BOHNING: That was a common tactic in academic institutions.

HURD: Two years afterwards, however, I was told to give it up and I would become the Clare Hamilton Hall Research Professor of Organic Chemistry for a considerable increase in pay.

[END OF TAPE, SIDE 3]

HURD: Clare Hamilton Hall was a former Northwestern chemistry major about 1900 that became an officer in the PPG <???> company. At that time, I was consulting for PPG's paint and varnish division up in Milwaukee. The head of it was Howard Gerhart, who was one of our Ph.D.s. He's the one who told me to ask one of our officers--I ultimately asked the vice president, Payson Wild--to approach the PPG people for some cash because they had given some to the University of Chicago that they were not going to renew. He thought that if they were approached, it might come to Northwestern. Which it did. Gerhart also saw to it that I should have a new title with some increase in pay, which was also a PPG grant based on that aspect of it. I had the idea it was going to be given to the department for departmental use, but in place of that it was given for a professorship.

BOHNING: We mentioned your book on the pyrolysis of carbon compounds earlier (7). That was 1929. Were you approached to do that?

HURD: No. I started that and then I asked Julius Stieglitz at the University of Chicago, who was the head of the committee that decided if those books would be ACS monographs. Apparently he was happy to do it. That was a long job of writing. [laughter]

BOHNING: Well, you've done a lot of that.

HURD: To actually do that, I started a course here for graduate students on the decomposition of compounds at high temperatures, which I didn't know anything about. From Monday at nine o'clock until Wednesday at eight o'clock, I was just having to work terribly hard in order to get what I was going to say on Wednesday! [laughter] But it was that kind of thing that made it possible to write the book.

BADER: When did you get into sugar chemistry? It's a long way from pyrolysis.

HURD: I was a consultant for Corn Products Refining Company, who actually gave the University some cash with which to do work on carbohydrates. That was given to me, and I could pick any topic I wanted to, providing it had something to do with carbohydrates. That is how I got into carbohydrates, with that money. That crass stuff, you know. [laughter]

BOHNING: Yes, you had your first paper in 1932 on pentose reactions (13), and the student that was working with you was a Quaker Oats fellow. He was supported by Quaker Oats.

HURD: His name was Lloyd Isenhour.

BOHNING: That same year you had a paper on the pyrolysis of furan (14). So you were still getting your pyrolysis work in.

HURD: I had some help from Quaker Oats, too. I was consulting for them, which didn't have anything to do at all with what I was working on. That is, I didn't do any problems here that were part of company problems. I'd just take a trip and talk to them on the company problem. But it was easier to get some cash to do some actual work, if they knew you and if you knew them. I asked for it indirectly a few times. For example, the Pabst Brewing Company gave me some money for carbohydrate work, too. That was because of Carl Miner. Miner was a consultant for Pabst, and he also was a consultant for Corn Products. He said, "Why don't you give Hurd some cash so he can keep on doing the work he's publishing. Thumb the pages and you'll see. This will help him do more." He was really a great friend and also a tennis player with a court up north of here at his home in Glencoe. I was invited to play up there pretty much, and we got to know each other better that way than just by being in chemistry.

BADER: I don't think Professor Hurd realized how much chemists in these companies looked forward to his coming, because in many companies--PPG, for instance--the chemistry was not particularly good. You were the one bright light. You would come and spend the whole day there and one could talk chemistry to you and you would understand.

HURD: As a matter of fact, that was true in quite a lot of companies.

BADER: I'm sure.

HURD: They ordinarily had sort of planned out in advance what we might take up. Then I'd be steered to various people to talk to on that idea. I'm sure that taught me as much as it taught them, if not more, because you have a perspective that you just don't ever get if you're not part of a company. In other words, what PPG was doing I wouldn't tell anybody else, but it would direct my thinking in the lecture that I was going to give. I think the companies knew they could talk to me freely. If they can't you're not much of a consultant.

BOHNING: You also had some work published in 1931 which was supported by [Thomas] Midgley and E. W. Webb of the Ethyl Corporation on the pyrolysis of lead compounds (15). How did you make that contact?

HURD: Well, by way of Thomas Midgley. I don't quite know how it started, but we really had quite a lengthy correspondence. Probably I could hunt my old notes if I haven't tossed them out, but he knew me and I knew him. He had some cash which he was able to direct my way. I didn't ever ask him for that, though.

BOHNING: Did that come about because there was some concern about public health with decomposition of tetraethyllead at that time?

HURD: I don't think so, but I just can't recall. Perhaps it was that we had organic mercury and organic lead work at Northwestern. I think it was more on that basis.

BOHNING: I also wanted to ask you about something that I had not known about until I read the paper you wrote about the Institute of Chemistry (16).

HURD: Oh, yes.

BOHNING: Whitmore ran the one here, I think, which was the second one.

HURD: Do you recall the name George Rosengarten?

BOHNING: Yes.

HURD: He was quite a wealthy industrial man in the East. Whitmore approached him to have something that would put our name forward. In other words, Whitmore was a booster trying to get people to realize that there was a chemistry department at Northwestern. He started this Institute of Chemistry in which he got some well known speakers to address the crowd that came during the time. For the second one he went to England to get Sir James Irvine here for the whole six weeks of the Institute. Well, that was really a scoop. Whitmore had to go to Scotland to get him, and he was happy of course, but it was not because of hubris. It was because he was boosting Northwestern. He was that kind of person. Lewis could never have done it, and I could never have done it.

BOHNING: Where did Whitmore's nickname, Rocky, come from?

HURD: Frank Clifford Whitmore--I haven't any idea.

BOHNING: I just wondered, because I've heard him called that.

HURD: That's right, Rocky Whitmore. Probably a Harvard nickname or something, but I don't know.

BOHNING: How long did that Institute of Chemistry last?

HURD: Two years. It simply died a natural death.

BOHNING: The program had an impressive list of speakers. Given the attempts of the ACS today to try and promote chemistry again, wasn't part of that Institute also public recognition of chemistry? Not just within chemistry circles, but wasn't it also to get public recognition of chemistry?

HURD: No, it was more to get public recognition of Northwestern University. That was what was in the back of his mind.

BOHNING: Did the department grow under his leadership?

HURD: Well, not tremendously in size, but it certainly did expand every year. It was sort of a slow development. But keep in mind it was zero about 1920. The thing that we actually had to do to get graduate students was get teaching assistant pay and fellowship pay. Once in a while, you'd get one who would pay his own way, but very rarely.

BOHNING: What kind of students were you able to attract?

HURD: Only the better ones. The best.

BADER: How many Ph.D. students did you have, all in all?

HURD: I'd have to add them up, but probably one hundred or so. All that I could do would be to go through my old notebooks and see what the names are, but the easier way would be to take the papers that I've got and read the titles and add them up. They aren't all in the doctoral category, but most of them are.

BADER: All of your graduate students did have papers, then.

HURD: Yes.

BADER: Because there are other cases like Bob Woodward where many worked with him and got their Ph.D., but no paper was published.

HURD: Well, I went the other way. In other words, I would try to make a paper out of a dissertation and put it under that guy's name with mine attached.

BADER: It's much fairer that way.

HURD: I had a purpose in putting my name first. Because if you want to hunt anything up, it's my name that people could find. If you have the others preceding in the index, you've got a terrible job.

BOHNING: It's very frustrating in the older Chemical Abstracts to see that. You look for someone, and it says "see:" and you have all these other names you have to look for.

HURD: As a matter of fact, I'm the one that had the problem they worked on in the first place. They just did the work. [laughter]

BOHNING: Did you assign problems or did you allow students to select problems?

HURD: I would offer probably three to choose from if they wanted to work with me, and they would pick which one of them they wanted to do. To have them come and tell me that they had a problem that they wanted to work on almost never happened. Not by my choice, but they don't have problems.

BADER: Did you have postdocs in those days?

HURD: A few, and some were pretty good. Some weren't so good. But I think there are more of that category now than there were then. Percentage wise, I mean.

BADER: Who was the best student you ever had?

HURD: Oh, there are half a dozen that I could name. One was Max Pollack. Another was Paul Austin, who as a matter of fact was not one of my Ph.D. boys. He took undergraduate work at Wisconsin-Madison. It was a poor bachelor's in chemistry because he thought about it too late. He arrived with really a bad preparation. He was here under me. That is, I had an application and I had some cash. He worked that year, but he couldn't pass the master's exam. He went to Cornell the next year, and I told him, "Next year at Cornell take it over again," which he did and he passed. But he's a pretty high class individual. Do you know about him?

BOHNING: No, I don't.

HURD: Well, I've got a paper here that I think I could find somewhere. He has a grant to do some work on clam shells. How's that for an organic chemist?

[the following addendum is excerpted from 7 November 1991 letter of C.D. Hurd to J.J. Bohning] Some of my other memorable students were [Oliver E.] Ted Edwards, Norman Kharasch, Charles Thomas and Hugh Anderson. I should also mention William A. Bonner, who went on to Stanford University; Donald Botteron, of Syracuse University; Ludwig Bauer of the University of Illinois at Chicago; and William H. Saunders, of the University of Rochester. They are all still alive.

Some of my other students went into industry, including Shin Hayao who worked for Miles Laboratories, first at Elkhart, Indiana and then transferred to be head of Miles Japan in Tokyo; S. Trofimenko of du Pont, who headed the du Pont headquarters in Warsaw, Poland for three years around 1980 and then returned to its large plant in Parkersburg, West Virginia; Louis K. Eilers, who was head of Eastman Kodak in Rochester, New York; and Elwood

Ensor, who was vice president of G.D. Searle and president of Searle International. Trofimenko is still alive, but Hayao, Eilers and Ensor died a few years ago. [end of letter]

BOHNING: One of the things I also wanted to talk to you about was your involvement with getting that organic model kit. It was born partly out of your frustration of teaching stereochemistry or three dimensional structures to students (17).

HURD: That's right. And because Toy Tinkers was here in Evanston as a company that made toys. I even had them spend about three weeks thinking how they could make them. But the problem was that all they knew how to do was to drill a hole straight through. They actually supplied us the pegs practically at cost,and they told us the person in Maine to hunt for to get the balls, because those were the balls that they used, except that they weren't put in properly. I actually had the original idea, but about a week later or so, Wallace Brode and Cecil Boord at Ohio State approached them on the same problem.

They called me up. I said, "I know them. Let me get in touch with them." They ultimately turned the project down. They said, "If you want to do it on your own, we'll be happy to hand the pegs to you practically at cost." (I don't think they wanted to lose money, but they didn't want to make money either.) Wallace Brode went to Maine and spent about a week there with the people that were drilling holes and taught them how to drill tetrahedral holes. [laughter] But it was a godsend in teaching. I don't have any idea what's happened to it. We actually kept it up as a company for quite a number of years. It didn't make money. Our idea was to have it be so cheap that you could insist that the students actually buy one. We priced it at about one dollar a box, and the profit, I think, was about two cents. [laughter]

BOHNING: How did you market this? Did you have a name for it?

HURD: Molecular Models. That was a name that was on every box. I have some yet.

BOHNING: Who handled all the paperwork and the ordering?

HURD: That was done at Columbus, Ohio by someone that they hired, I guess. The cost was split three ways, or at least I assume it was. At least I was paid something when they told me how much it was.

BOHNING: That's the same thing that Sargent took over in about 1949.

HURD: You have actually seen that box, haven't you?

BOHNING: I have a Sargent box from years ago that I picked up.

HURD: An oblong one, about an inch high? That's probably exactly the same box, providing it had printed on it the word Molecular Models.

BOHNING: The one I have is a Sargent one, so it's later than the early one. Did you tell your students they had to buy one of your kits?

HURD: I certainly did, and what made that profitable was because of the large number of students at Ohio State. Our students had to have it here, but our numbers are trivial compared to them, because we don't have agriculture, we don't have the big engineering courses that they do, and we don't have very many chemistry students compared to them. Ours added to it, but what they had was a big bunch of sales. We purposely devised some experiments to do in the laboratory with those boxes of models. In other words, we insisted that they do it and by observation in the lab see to it that they did. We hoped that it would help them with the textbook. But I just didn't ever have any problem with stereochemistry after that.

[doorbell rings, tape interrupted]

HURD: [discussing flyer announcing the Charles D. Hurd Scholarships for excellence in undergraduate research at Northwestern] They were to appoint two scholars for senior research at Northwestern bearing my name. That is, all of the departments that are interested will put in the name of the best candidate that they are offering. They will pass it on and the committee will pick two out of that list.

BADER: What you didn't mention is that you are supplying the funds.

HURD: Well, part of it, anyway.

BOHNING: We were talking about those Molecular Model kits. Did you sell them to other institutions? Did you advertise in Journal of Chemical Education?

HURD: I don't think we ever advertised. I think there was correspondence with professors that we knew. We told them that if they were interested to let us know and we'd send them out to them. The details of that were pretty much with Boord and Brode. I'm happy I didn't have it.

BADER: How did they compare with the Fieser models? Louis Fieser also wanted them as cheaply as possible.

HURD: These were first. After you put out something, then some other people can improve on it. You have seen these, haven't you? These old molecular models? [Hurd gets a box of models]

BADER: Well, in those days Aldrich couldn't sell them because Aldrich didn't exist.

BOHNING: This one still says, "Manufactured by Molecular Models."

HURD: Yes, but this was after we got out of it. I don't have any idea what has happened to them now.

BOHNING: I think you can still buy them. I think they're still in the Sargent catalog.

HURD: I'm going to have to ask some of our teachers if they ever use them anymore or what do they use. As a teaching device it was a godsend to me to have this for carbohydrates and proteins.

BOHNING: How did you get Sargent to take over?

HURD: I think they asked us, but I'm not positive about that either.

[END OF TAPE, SIDE 4]

BOHNING: I have a long list of things here in terms of the chemistry that you have done. In keeping with this more general line, you were listed as one of the cofounders of the Journal of Organic Chemistry, and you wrote something about [Morris S.] Kharasch (18).

HURD: Here's the article that ought to have a reference on it. In essence, I actually raised the question here, who started it? I came to the conclusion that it couldn't be anybody else but M. S. Kharasch. He put some cash in it and all those things, and then he apparently invited the people that would be on the editorial board. I was one of them. Starting it, however, one hundred percent was Morris Kharasch. [to Bohning] You can't have that. [laughter] You can look at it.

BOHNING: What was your role, though, beyond being just one on this list of people?

HURD: Once in a while we'd have editorial board meetings in those early years at a time when there was a convention. I think I was at about three or so of them. [Lyndon F.] Small became the editor after the first one, and I think the first editor was [Otto] Reinmuth.

I think Kharasch felt that there was only one place in the country that you could get organic papers published, and that was in the Journal of the American Chemical Society. He thought that was bad, and decided to do something about it. I know he tried hard to get Roger Adams to do it, but Adams said no. He didn't want to have that kind of work. But a person that was important on it was Marvel, who was practically Adams' right hand man. It was just simply an outlet for organic papers of which up until that time there was only one and he thought there ought to be at least two.

BOHNING: This wasn't published by the ACS first, was it?

HURD: No, no, no. This had nothing to do with the ACS.

BOHNING: Did the University of Chicago Press publish it? It was somewhere in Chicago, wasn't it?

HURD: I can't answer the question. Publication wasn't one of my worries, just the address where to mail it was.

BOHNING: You also had one of his nephews as a graduate student, didn't you (19)?

HURD: Yes.

BOHNING: Was that through your connection with him?

HURD: Probably. That is, Morris and I were pretty good friends. The one who worked with me was named Norman. He's out in California now. He was going to get his Ph.D. at the University of Chicago, but Morris said, "No you're not." So he's the one that aimed him up here. I think he kind of felt that Chicago would not be a good place for his nephew to be, as if they might talk favoritism or something like that. But he was a pretty good man in his own right.

BOHNING: In all the years that you were teaching, what were the major changes that you saw in the way in which you taught organic chemistry?

HURD: I don't know that I had any. Probably if I'd teach it today I'd teach it the same way I did then. [laughter] Obviously, if there was something important that was new, you'd try to work that in, but you'd always have to take out something else. And you could mention the thing incidentally very often. For example, in penicillin, I could have spent a month on that, or an hour. Well, I put in an hour. There's a carbon up there with a sulfur and a nitrogen going off from it, and that's all. All the rest were carbon bonds or hydrogen bonds. Well, I'd ask them, "What do you think about that?" "What does it look like to you?" I was hoping that somebody would say, "Aldehydes." Because that's the way an aldehyde structure is. Two of the four bonds go to something that's negative. And, I told them, "If you know that, you can predict that's where it ought to hydrolyze, can't you?" Well, it's that kind of thing. If you can weave the fundamental story into a particular structure, the kids began to get the handle to it and then that puts them in a place where they can do a lot more with it. All of them are very interested in seeing if they can be taught something about things that are in the papers these days. The papers don't do a very good job of it. [laughter]

The papers, incidentally, are beginning to get a lot of criticism--or at least the chemists think that the papers are treating them unjustly. I recall that Roald Hoffmann in Hawaii about 1988 talked about "chemophobia" that's coming over because of the press. That's the word he used. About two months after that, in C&EN [Chemical and Engineering News] there was a person who had analyzed a large number of newspapers for how they

treated the word "chemical." About eighty to eighty-five percent of the time, he said, it said "damaging" or "dangerous." In other words, in the press, chemistry is not given a good name. Well, it doesn't make news. News is if you've had somebody eat some poison or something.

I've just completed putting an article out that I think I may pass to C&EN to see if they won't run it. It would need a large publication. The idea is that a chemist has to talk to a non-chemist if you're going to improve something about chemistry. Well, what do you say? If we start with the vitamins or something like that, you just can't make it interesting if they don't know any chemistry. That's all the papers do. If that's all that chemists could do, it's stupid. So I've got a paper coming out that I'm putting a title on--"The Other Amadeus" (20). How we can let Avogadro help the image of chemistry. It was Amadeo Avogadro. Amadeo is the Italian equivalent of Amadeus in Austrian. Of course, I point out that the first Amadeus is Wolfgang Amadeus Mozart that everybody knows because of Peter Shaffer's play and subsequent film, "Amadeus." But I think Amadeo Avogadro is one of the greatest chemists we've ever had, because he's the one that told people how to get molecular weights accurately. You can tell it to a non-chemist perfectly, by just defining the word element, atom, and molecule. Avogadro was talking about molecules. You can point out that the same molecules of hydrogen and oxygen in the same volume will weigh differently, but they've got the same number. Then I've developed that to talk about air.

I think that this may interest you. A weather prophet on TV will tell you that if the barometer is going to get lower, it increases the chance of rain. But he doesn't ever tell why. If you'd ask him why, he'd probably say, "Because that's what I was taught in school." Which of course doesn't say anything about why. I developed the point that a barometer measures atmospheric pressure and a lower reading has to mean that something in the air has given rise to the lower pressure. That something usually is water vapor (gaseous water) which weighs less than air. It weighs 18 compared to air that weighs 29. (In other words, 28 is N_2 and 32 is O_2, and air is about a quarter oxygen. So 29 is roughly what you can use with air to do that.) If you'd have a bowl in your kitchen that was dirty and you washed it and put it upside down in the rack to drain, after it's drained out you'd leave it that way or else you'd turn it right side up. That's in your kitchen, now. How could Amadeo Avogadro help you on that? You turn it right side up because now you're talking about gaseous water rather than liquid water. It has an evaporate with a molecular weight of 18. A thing like that is thoroughly understandable to a non-chemist. If you're going to talk to a non-chemist you've got to be pretty careful, or you'll get into a subject where you just can't talk about it because it's too complex.

I think that's the way to improve the image of chemistry, for a chemist to talk to a non-chemist. [to Bader] You could talk about your company, in a way that is chemical, but interesting. I wouldn't be able to, but you would because of your knowledge. Have you also thought that the papers are doing a bum job for chemists?

BADER: Absolutely. It's terrible. Not too long ago there were headlines in the Milwaukee Journal that they found two parts per billion of benzene in a well. [laughter]

HURD: Yes, yes. And that gets into the air.

BADER: Yes.

HURD: As a chemical.

ISABEL BADER: The advertising for food products is--"all natural," "no chemicals," and this kind of thing. "Potatoes--no cholesterol, no fat." But that's the way to sell.

BOHNING: You did a lot of writing for encyclopedias. I'm just following up what you're saying now, because you were doing that in a certain sense by writing for encyclopedias. How did you make that first connection?

HURD: I think it was through Austin Patterson, who was a chemical historian, more or less, and writer. We actually knew each other pretty well. The biggest job wasn't writing for encyclopedias, it was the dictionary (21).

BOHNING: I was going to ask you about that.

HURD: I had done that type of thing for the small Random House dictionary that came out earlier (22), and then Merriam-Webster asked me if I would do it for their third new international dictionary. I thought it might be a trifle harder than the other ones, so I put the price up a bit, up to three thousand dollars. It was the hardest three thousand dollars I ever earned! [laughter] I thought it would take me my spare time for a year; it took it for ten years. [laughter]

BOHNING: Weren't there about twenty thousand terms that you worked on? How did you manage that?

HURD: They actually sent me sixty steel boxes about 18 to 20" long that would hold 3x5 cards. Cards is a sorry term. Half of them were on India paper. [laughter] There were a lot of cards there also. For example, the word acid might have about a foot of cards. I'd have to define the word acid by considering every one of those cards, because it has various meanings. I would take those cards on a consulting trip, on the train or airplane, and I'd take about the number that I thought maybe I could handle on the trip. Up at Ephraim, Wisconsin, where I have a summer place, I'd take those cards up there in one of these boxes and do it up there. But it was spare time work that was very, very long.

BADER: Do you still go to Ephraim every summer?

HURD: Well, two years ago I was not there. That's the first time I've missed. I was there for a very brief time this year. You see, I am up there by invitation now. I don't own it anymore. I gave it to three people, my son and his two daughters. It'd be much more difficult if I hadn't done that while Mary was alive. That was easy to do then. Now I'm discovering that with her gone about a year now, there's a trust end to the will and it's impossible to do that without a lawyer. [laughter] Apparently it does save tremendously on the person that actually gets the stocks or money or whatnot afterwards.

BOHNING: We were talking earlier about the different kinds of support that you had from the API and from Quaker Oats and Corn Products, but during World War II, you did work for the Quartermaster Corps. You also did some mustard gas work for the Chemical Corps.

HURD: That's right.

BOHNING: After World War II started, how did that happen? Did they come to you because of your previous work?

HURD: Well, I think it happened because I was in a group at a chemical meeting where there were quite a number of chemists who were well enough known to direct some projects. I was in that group, and we all more or less applied at the same time to the same people. We'd have some cash allocated to us with which we could hire some persons to do that kind of work. That's all there was to it. The war was on and this was a group that thought that the chemists ought to have some participation in the research work that was underway. I hope it helped; I don't know.

BOHNING: Was that work done up here at Northwestern?

HURD: It was here, and it was private. In other words, that was one of the things I could not talk about. I would not have ever taken it at all in peacetime. After the war was over, I was asked to continue but it was on a basis that I actually could publish.

BOHNING: I was wondering about that because some of the papers were published in the late 1940s. It was five or six years after the war was over before some of these papers were published. But that wasn't necessarily the work that went on during the war but from the continuation after the war.

HURD: Perhaps it was even there during the war, too, because many of those papers after the war was over were cleared for publication. But they had to be cleared because the promise was you weren't going to talk about it.

BOHNING: Did you have graduate students working on those problems?

HURD: I think mine were all postdocs.

BOHNING: One of the things that strikes me as I look down your list of publications is the incredible variety of work that you did.

HURD: Incredible? [laughter]

BOHNING: I'd like to talk about a few of these.

HURD: "Toy Balloons and Filtration" (23)? [laughter]

BOHNING: Yes, "Toy Balloons and Filtration"!

HURD: I have a feeling that I did not expand that into ten papers. [laughter]

BOHNING: You started out in pyrolysis and then you were in carbohydrates.

HURD: No, I started out in ketenes.

BOHNING: Ketenes, okay. Then pyrolysis.

HURD: Then pyrolysis. But, you see one thing led to another. Ketenes was a lot of high-temperature stuff I had to work out and discover. If you can heat acetone, why can't you heat ethyl acetate or something? It was just sort of an ordinary development after that. As we've indicated, carbohydrates came by a grant, with the help of Mr. Miner.

BADER: Yes, but sugar chemistry and pyrolysis are about as different as two fields could be.

HURD: Oh, no. I've got a paper on thermal decomposition of sugars with the title "Thermal Dehydration of Sugars." (24) [laughter]

BADER: Somebody burnt it on a cake.

HURD: But you are, however, correct, as always.

BADER: But you see, there was one school of sugar chemistry at Ohio State that really concentrated on it. And then, you came along.

HURD: But the aspect that I had was entirely different.

BADER: Sure it was.

HURD: It was to treat carbohydrates not as natural products but as organic compounds. There were a whole lot of reactions that had been completely skipped.

BADER: Now they're chiral building blocks.

HURD: Yes.

BOHNING: Let me just mention a few others then. You had a paper on carbon suboxide, which said you had a better method for making carbon suboxide (25).

HURD: Well, that's a ketene.

BOHNING: Bleaching powders and ketones (26).

HURD: That was with Charles L. Thomas, whom I just got an old communication from this last week. I think it was because of some levulinic acid that I got tangled up with.

BADER: From Quaker Oats?

HURD: From Quaker Oats. I knew that it was going to become commercially available, and so I began some projects in levulinic acid, and probably the bleaching powder came indirectly as a result of that one.

BADER: Remember Johnson Wax behind the patent on levulinic acid?

HURD: I was purposely careful not to tell the president of it, who had also a place up at Ephraim; I knew him. [laughter]

BADER: It was so funny because as a salesman from Quaker Oats was going north--Johnson Wax, Milwaukee, 3M--and within a week or ten days he told three of us and here was a ketone and motherhood to make up the bis-phenol from it. Then there was a long legal fight.

HURD: Yes.

BOHNING: You did some work on the ozonolysis of triple bonds (27).

HURD: It was impressive to me because I was using ozone for different purposes and then I happened to realize that this had never been done, so I did it.

BOHNING: You did something on the structure of cyclooctatetraene (28).

HURD: Yes. But not much.

BADER: Was this in connection with repeating [Arthur C.] Cope's work?

HURD: I don't know whether he or I were the first to get in print on that, but the two projects were different and I don't recall too much of what I actually did. It was not very important in my work.

BADER: Cope repeated [Richard] Willstätter's work (29). [Hugh S.] Taylor had published in the early 1940s (30), thinking that Willstätter never had it.

HURD: I actually began it from that point of view, to find out if I could get substantiation or not, and I think I was able to prove completely that he had it.

BADER: Yes. But unbeknownst to you, they made many kilos at Badische (BASF).

BOHNING: In 1941 you published a paper on the structure of diketene from spectroscopic evidence, and you measured absorption spectra (31). I was curious how you measured the absorption spectra. Do you remember what type of instrument you used? You were down into the UV when you were looking at that.

HURD: We had a spectrophotometer at the university.

BOHNING: The reason I'm asking is that this is the 50th anniversary of the [Beckman] DU coming up. It was unveiled in 1941, and you had published a paper actually prior to the time the DU was available, so I was wondering what kind of instrumentation you had to measure UV spectra at that time?

HURD: Well, I can't tell you. If you'd ask a physical chemist over there, they could answer it, because that's the way I picked the instrument up, with some instructions as to what to do.

BADER: Well, until the early 1950s there was doubt whether the double bond of diketene was inside the ring or outside. Until Gutowsky and Bader published the NMR spectrum (32). It was the first use of NMR to determine the structure of a simple molecule.

BOHNING: You also had a patent on chloroisoprene in 1942 (33).

HURD: Good for me. [laughter]

BOHNING: It was with the Commercial Solvents Company. Were you doing work for them?

HURD: I was a consultant there, yes. And the patent was to them. Of course, there is no such company now. It was taken over by IMC [International Minerals & Chemical Corporation] with their Pitman-Moore division at Terre Haute.

[END OF TAPE, SIDE 5]

BOHNING: Not too many patents came out of your consulting work.

HURD: No. There was a total of maybe ten or twelve (34). There is a patent that I should have taken out.

BOHNING: Oh, really?

HURD: Making ketene from acetone.

BADER: Union Carbide would have to pay you a lot of money. [laughter]

HURD: Well, that's the way acid anhydrides were made. Union Carbide said, "Well, it looks as though this guy Hurd didn't patent this, did he?" [laughter]

BOHNING: Had you thought about patents in those early days?

HURD: No. A patent was a complete stranger to me.

BOHNING: There were probably no patent agreements with the university in those early days.

HURD: If there is anything patented here now, the university wanted to be the one that it's actually assigned to.

BOHNING: Right. But in the days when you came, that didn't exist.

HURD: No, no. I didn't have any patents at all that I'd patented on anything that I was doing privately. It's only these industrial contacts where I had an original idea, therefore, the patent was in my name. But if there's any profit, they get the profits. I don't think there are ever very many profits. [laughter] It's about a hundred patents to one that pans out, isn't it?

BADER: Sure.

BOHNING: In the early 1950s you did some deuterium tracer work in pyrolysis, too (35). That was pretty early to be using deuterium tracers, wasn't it?

HURD: Well, I helped on that because at that time a professor here, Malcolm Dole, was doing work on isotopes. (Incidentally, he died only a few months ago.) He was probably the most important physical chemist we had here and a very nice fellow. I'm the godfather of his son, and for a Baptist that is something--we don't have that word. So he and I thought pretty well of each other.

BOHNING: I know you had a paper with Arthur Frost (36). I was going to ask you about Frost, because I learned my kinetics from Frost and [Ralph] Pearson (37).

HURD: He's in Arizona, and Pearson's in California. Both of them are alive yet, and both very smart boys in the mathematical part of chemistry.

BADER: Is Max Pollack still living?

HURD: Oh, yes. He is in the metropolitan New York area of New Jersey. I think it's Morris Plains where he lives. He has a very charming wife, also alive.

BADER: He is also a very good chemist.

HURD: He is. Do you know him?

BADER: I know of him.

HURD: Yes. He actually did some tremendous work with me here (38). There's a person from Canada that did very good work with me, too (39). Oliver Edward Edwards--Ted Edwards.

BADER: He is retired now from Carlton University.

HURD: That's right.

BADER: We visit him once a year. He worked with Marion on Sussex Drive, you know, National Research Council.

HURD: He is a very nice fellow and a good violin player.

BADER: Is that right?

HURD: Yes. But isn't he up there yet at Carlton doing some private research?

BADER: Yes, private.

HURD: Which means he isn't retired except on the payroll.

BADER: Well, he's got a very nice lab on the fifth floor with [Dr. J. W.] Apsimon. Did you have many Canadians?

HURD: There's one at Nova Scotia now. And Edwards. No, I did not have many, but I had some.

BADER: Who was the one at Nova Scotia?

HURD: I know it as well as I know my own name, but right now I just can't think of it. [Hugh J. Anderson, a professor at Memorial University, St. Johns, Newfoundland] I think he's going to come and call on me here this June or so. He's got an aunt that lives in this area. Doesn't that drive you nuts? Wait until you get to be ninety-three. [laughter]

ISABEL BADER: I don't have to wait. [laughter] I can't remember now.

BOHNING: You did collaborate a lot with other people in the department. We've mentioned the paper with Frost (36), and you had one with Irving Klotz (40), and you did the deuterium tracer work (35), so you were really quite involved with your colleagues here.

HURD: Well, you have to be broad minded. [laughter]

BOHNING: Can you tell me any more about how the department has changed over the years?

HURD: I'm over there at least every Tuesday noon for a bag lunch, and I think that's the reason we've got such a good department. We've been sort of choosy in whom we hire, and if we didn't think they'd grow, we would tell them so in the first three or four years they were here, and they could look elsewhere. They would not get promotions here. But, we've only had about one or two of those. What the bag lunch does is, one person on the faculty is talking to the rest of the faculty telling what he's doing. All the faculty is there, physical, organic, analytical, bio (except bio now is pretty much absent--they're their own department now). But telling what they're doing and knowing that their time is coming up sometime during the year means that they've got to have something to say. I'm impressed that we've got a very, very good department.

[Fred] Basolo has brought the inorganic group a long way. When he started out, inorganic chemistry here was pretty thin. I think it's probably the biggest of our divisions now over there. Honestly, it's a powerhouse we've got over there in inorganic. I don't mean that organic is poor, but I think that the inorganic is better. With [James A.] Ibers and [Richard P.] Van Duyne, that is a group over there that I don't think can be matched.

BADER: It keeps you young, anyway.

HURD: Yes.

BADER: How was the lecture last week? [1991 Charles D. Hurd Lectures] I'm sorry I had to miss it; I had an important meeting.

HURD: Well, he [Robert M. Nowak, Chief Scientist, The Dow Chemical Company] brought out the fact that The Dow Chemical Company's pretty big. [laughter] I hadn't realized that they were the biggest producer of polyethylene and polystyrene in the world, which isn't bad. But he pointed out that there's other

things where they have plastics do the work of metals. They even have a plastic automobile engine. He indicated in the talk, that one of the polymers that he had kept at 350° for a period of time happened to lose about 1.3 percent of its weight. I talked to him at the end of the lecture. "You actually said that this polymer lost that much on heating to 350°, but you make an automobile engine?" "Well," he said, "that also does lose weight." [laughter]

BADER: Really. I'm afraid they have treated chemists worse than any other company.

HURD: But they're putting out automobile bodies of plastic that are gorgeous things and, I guess, is a pretty big business for them. At least they were pure white. I asked him afterwards also, "You indicated these automobiles were made out of carbon polymer but are pure white? I thought carbon was black." "Well," he said, "it actually is, but we put epoxy coating on the top of it that's white." I was impressed that he had really quite a lot of good stuff to say. It's an impressive company.

BOHNING: You were very active in the Chicago ACS Section.

HURD: Oh, for a period of time.

BOHNING: And you wrote for The Chemical Bulletin for many years.

HURD: Yes. It's always been an active section, and I'm positive it is yet, but I don't take evening trips anymore and I'd probably get lost in the loop in Chicago. [laughter]

BADER: You're not alone. [laughter]

BOHNING: Well, we've talked about your nomenclature work. In the paper in Journal of Chemical Education (41) you mentioned the problems you had with C. S. Hudson.

HURD: He was a stinker. He was a genius in carbohydrate science, but his particular problem was [Horace S.] Isbell. Isbell is a chemist who worked with Hudson for a while. Hudson had put out some rules for carbohydrate chemistry, and Isbell offered to improve on the wording of one. Hudson didn't want to have anybody improve him. So, he just got down to something terrible. Isbell is prominent in carbohydrate chemistry; he worked at the National Bureau of Standards. He became the

chairman of the carbohydrate division and organized a symposium on carbohydrate chemistry. He asked me to give a talk on carbohydrate terminology. There was a committee formed for carbohydrate nomenclature after that talk, and Isbell appointed me to it (he was the chairman). The fact that Isbell had anything to do with it put Hudson also against me if I stayed on that committee. It's just incredible. He and I had always been compatible; he came twice, as a matter of fact, to see the carbohydrate work that I was doing here. But the instant that I stayed on anything that Isbell was on, my name was nuts. But I was glad to be able to tell people that, because Isbell couldn't.

BOHNING: I imagine that would have been difficult for him.

HURD: Yes. Both Isbell and R. S. Tipson were involved, and both of them gave me a tremendous "thank you" after my talk. They said, "Well, we both stood up to the great man, didn't we?" [laughter] Absolutely pointless.

BOHNING: It must be difficult when you have a man whose chemistry is so good.

HURD: He was just like an eight-year-old boy who can't have his own way.

BOHNING: Is that an unusual event in your long career of interacting with people?

HURD: He's the only one. Ordinarily you can converse with people that you know something about, and pleasantly; it's always been that way.

BADER: You should read the papers of [Adolf W. H.] Kolbe who was against [Friedrich August] Kekulé. "How can they get an idiot like that rector in Bonn?" [laughter]

BOHNING: Is there anything else that you would like to add at this point?

HURD: I can indicate one thing that perhaps you aren't aware of. My protein work is in addition to my work on molecular rearrangements. If you have a carbonic hydrazide, it will rearrange in the same way and you can make polypeptides that way, a way that does not start with an amino acid. It starts with the

half ester of a malonic acid that you convert to a hydroxamic acid. It's a kind of an ingenious way to do it, but it's an outgrowth of that work on hydroxamic rearrangements.

I don't know of anything else that's of any interest, except that I haven't worked my crocus pods out there yet. [laughter]

BOHNING: Dr. Bader, is there anything you'd like to add before we close?

HURD: You came up here to hold your wife's hand, I can see. [laughter]

BADER: When you've been the admirer of a man for forty-one years you don't want to miss the opportunity. I can't tell you how I looked forward to your visits to Milwaukee. You were the one man I could talk chemistry to. Earl Parker I couldn't talk to.

HURD: No. Well, he was about the best one otherwise, but he wasn't in your category. But PPG was very helpful to Northwestern.

BADER: They were very helpful to me. [laughter]

BOHNING: With that I'd like to thank you very much for spending the morning. I've enjoyed it immensely.

HURD: Well, I've been very glad to meet you. May I take your picture in the middle between those two charming people? [laughter]

[END OF TAPE, SIDE 6]

NOTES

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